of the gases on the enclosing wall. Kirchhoff deduced the consequences of this theory mathematically, and pointed out that the diminution of velocity must vary inversely as the square root of the number of vibrations in the sound propagated; that is to say, that the velocity of sound could not be uniform for different notes. Seebeck shows that the loss is inversely as the cube of the square root of the number of vibrations; so that we can scarcely attribute the result entirely to the loss of heat by friction of the enclosed gas on the walls of the tube. On the other hand, his results completely establish the fact of a difference in the speed with which different notes of music are propagated in tubes.

The magnificent and laborious work of M. Regnault on the velocity of sound in pipes, appeared while Seebeck was occupied in these investigations. He points out, that since the sounds which Regnault studied, such as explosions of gunpowder and so on, are due to violent and complicated disturbances, however important it may be to study them in a practical view, they are not likely to give us accurate and delicately differenced results. Where the sound contains a mass of irregular notes, no effect due to difference of note can be observed. The method of generation compels the air to move at first in all directions in the tube. Such sounds fall into the sea of air like a mass of stone dropped from a great height into the water. The waves which they generate can only reduce themselves to regularity at some distance from the disturbance and their propagation must be irregular till that distance is reached. On the other hand, a regular pulsation, such as would be produced by the timed advance and retrogression of a piston at the end of the cylinder, gives waves which are regular and regularly propagated from the beginning. It is in consequence of this delicacy in the character of his experiments that Seebeck has attained his results.

He makes one of König's tuning-forks sound at the open mouth of a cylindrical pipe, the other end of which is stopped by a moveable piston. A short distance from the mouth of this pipe there is an opening, connected with an indiarubber tube which can be carried to the ear. If, by the motion of the moveable piston, this opening be made to correspond exactly to a "ventre" in the standing wave which is generated in the air of the tube, it is easy to measure, with great accuracy, the length of that standing wave, which corresponds to the precise number of vibrations of the tuning-fork. The extreme difference in fifty experiments for the same note, appears to have been about one five-hundredth of the whole amount.

The results are the following, the velocities being reduced from the experiments to those at 0° C.:

	Diameter of pipe in millimetres.	Velocities reduced to 0°, in metres per second.				Number of vibrations per second in note sounded.
1.	3·4	322·90	318·86	317·26	——	512, 384, 320, 256
2.	9·0	328·44	327·68	327·22	325·63	
3.	17·5	330·92	329·86	329·24	327·82	
4.	29·0	326·10	326·72	325·36	324·54	

If we compare these numbers with that for the speed of propagation in free space, as given by Schröder von der Kolk, which is 332·77, we see (1) that they are *uniformly* and considerably *less*; (2) the divergence *is greatest in narrow tubes*, with the exception of that where the tube was 29 millimetres in diameter, which was in all probability too wide to have the mass of air at its end uniformly affected by the vibrating tuning-fork; (3) the notes which travel slowest are the *low notes*. This difference is by far the greatest in the case of the narrow tube of an eighth of an inch in diameter, amounting to nearly 1 in 60 of the whole amount for the notes e and c_2.

The author discusses the further question, whether the nature of the enclosing tube produces any effect. In a tube, the inside of which is sheet copper, which has nearly 17 mm. diameter, the velocity is as low as it is in a glass tube of 9 mm. Where the friction is raised to a maximum by coating the inside wall of the tube with flannel, the velocity is reduced to 293·7 metres per second in a tube of 13 mm. diameter.

The same subject is treated fully by Herr Schneebeli, in a series of experiments published in Poggendorff's *Annalen* in February of last year; but those which we have described appear to us to have conducted their author to more valuable and interesting results.

WILLIAM JACK

An Introduction to the Study of Chymistry. Written for the People by Cuthbert C. Grundy. Pp. 108. (London: Simpkin, Marshall, and Co.)

WE remember when at school being called upon to admire the beauties of Schiller's *Wilhelm Tell*. Our impressions of that play were then by no means complimentary to some of the principal personages: so far as we could see Melcthal, Stauffacher and the rest of the members of the three Cantons apparently did nothing but meet clandestinely (when the weather was favourable) to talk much treason and fine sentiment, generously leaving most of the hard work to be performed by Tell (who being lowly-born was perhaps not so free of speech). All this was doubtless wrong and absurd; but we are reminded of these early impressions by the character of much of what is being said and done in reference to the education of the masses in the present movements. There is a wonderful disparity in the proportion of Tells to our ideal Melcthals and Stauffachers. Possibly, if certain noisy persons would but show their earnestness in the practical manner of Mr. Grundy, the wheels of progress would not drag so heavily.

In a modest little preface, Mr. Grundy informs us that his book is mainly intended for those among the great body of the people who desire knowledge, but have little time and only limited means for acquiring it. Occasionally, for example, when defining and illustrating the phenomena of latent heat, the author lays himself open to the charge of sacrificing precision of knowledge to clearness of statement. This fault is not unfrequently met with in manuals of this character: surely the two are not incompatible. Why does Mr. Grundy prefer the more antiquated form of styling the science? The generally accepted word is undoubtedly more correct: Kopp has satisfactorily shown this in his recently published "Beiträge zur Geschichte der Chemie."

On the whole, however, we can congratulate Mr. Grundy on having succeeded in explaining in clear and simple language the fundamental principles of chemical philosophy.

T. E. T.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

On Professor Tyndall's Exposition of Helmholtz's Theory of Musical Consonance

IN the course of a re-perusal of Helmholtz's "Ton-empfindungen," it occurred to me to compare the theory of consonance and dissonance, there propounded, with the exposition of that theory presented in the Lectures on Sound of Professor Tyndall. The result of the comparison is the present paper, in which I shall endeavour to show that Professor Tyndall's version of the theory is radically different from the original, and erroneous.

Helmholtz determines the consonances of two *simple* tones by reference to their combination-tones. That of the first order suffices for the *octave*; those of the first and second order determine the *fifth* and *fourth*. The remaining consonances—*major and minor sixth* and *major and minor third*, do not, according to him, admit of determination from two simple tones and their combination-tones, but require the addition of a third primary simple tone.

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* "Ton-empfindungen," pp. 301-303, 306-307. I shall in the following references use "H." to denote this work, and "T." for Prof. Tyndall's lectures.

Professor Tyndall[*] undertakes to determine the consonances of the *octave*, *fifth*, *fourth*, and *major third* for two simple tones, without employing combination-tones. He writes as follows :—

"Bearing in mind that the beats and the dissonance vanish when the difference of the two rates of vibration is 0; that the dissonance is at its maximum when the beats number thirty-three per second; that it lessens gradually afterwards and entirely disappears when the beats amount to 132 per second—we will analyse the sounds of our forks,[†] beginning with the *octave*. Here our rates of vibration are—

$512-256$; difference $=256$.

It is plain that in this case we can have no beats, the difference being too high to admit of them.

"Let us now take the *fifth*. Here the rates of vibration are—

$384-256$; difference $=128$.

This difference is barely under the number 132, at which the beats vanish; consequently the roughness must be very slight indeed.

"Taking the *fourths*, the numbers are—

$384-288$; difference $=96$.

Here we are clearly within the limit where the beats vanish, the consequent roughness being quite sensible.

"Taking the *major-third*, the numbers are—

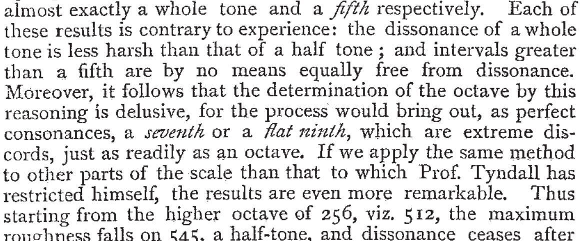
$320-256$: difference $=64$.

Here we are still further within the limit, and accordingly the roughness is more perceptible. Thus we see that the deportment of our four tuning-forks is entirely in accordance with the explanation which assigns the dissonance to beats."[‡]

It will not be difficult to test the value of the above reasoning. Starting from the rate of 256 vibrations per second selected by Prof. Tyndall, all that can be deduced from his definition of beats and dissonance at the head of the extract is that the maximum of dissonance will fall on the interval $256:256+33$, *i.e.* $256:289$; and that all intervals larger than $256:256+132$, *i.e.* $256:388$, will be free from dissonance. These numbers indicate almost exactly a whole tone and a *fifth* respectively. Each of these results is contrary to experience: the dissonance of a whole tone is less harsh than that of a half tone; and intervals greater than a fifth are by no means equally free from dissonance. Moreover, it follows that the determination of the octave by this reasoning is delusive, for the process would bring out, as perfect consonances, a *seventh* or a *flat ninth*, which are extreme discords, just as readily as an octave. If we apply the same method to other parts of the scale than that to which Prof. Tyndall has restricted himself, the results are even more remarkable. Thus starting from the higher octave of 256, viz. 512, the maximum roughness falls on 545, a half-tone, and dissonance ceases after 644, which lies between a *major third* and a *fourth*. For the next octave, *i.e.* starting from 1,024, dissonance ceases before we reach the interval of a whole tone. If we take lower positions on the scale we obtain opposite results. With 128 as our fundamental note, the maximum dissonance falls on 161, slightly above a *major third*, while roughness extends to 260, just beyond an *octave*.

With 64 the worst discord is at 97, just above the *fifth*, and roughness reaches 196, another *octave* higher. Starting from 32 the worst dissonance 65 is just above the *octave*, and roughness is not got rid of until 164, *two octaves and a major third* from the fundamental note.

The following octave exhibits at one view the results we have reached. The lowest note of each triad represents the fundamental note, the middle one the position of maximum dissonance, and the highest the limit of roughness. The middle note necessarily falls out of the last triad, as it lies too near the fundamental note to be represented.



Prof. Tyndall's method leads to the following conclusions :— The interval of an *octave* from the 16-foot C is the harshest possible dissonance; so is that of a *fifth* from the 8-foot C; so is that of a *major-third* from the lower C of a baritone voice. On the octave above the high C of a soprano voice, all the intervals beyond D are perfect consonances, while in the 16-foot octave there are no perfect consonances at all.

These conclusions are so utterly at variance with facts, that the method by which they have been obtained must be pronounced erroneous. In fact, Prof. Tyndall is himself a witness that this is so; for, in speaking of the *octave*, he remarks that if this interval be slightly impure, *beats of the fundamental tone are heard.*[*] Now this does not square with his own theory; for suppose two simple tones with 513 and 256 vibrations per second, which would form an impure octave : the difference is 257, which is as much "too high" as 256 was in the case of the pure octave. This interval should, therefore, give no beats, and an impure octave be as harmonious as a pure one. But according to Helmholtz's view, the first combination-tone of 513 and 256, viz. 257, will produce one beat per second *with the fundamental tone*, as stated, but not satisfactorily explained[†] by Professor Tyndall. This amounts to a practical admission by him that the beats of two simple primaries are not adequate for the determination of their consonances, and that recourse must be had for this purpose to combination-tones.

I claim to have shown that the method by which Prof. Tyndall appears to determine the consonances of simple tones is erroneous, and the determinations themselves fallacious. I proceed to point out the defect which vitiates his reasoning. He enunciates but one condition for the production of audible beats, that their number should not exceed 132 per second. Helmholtz lays down a second, quite as important—that the tones producing them should not differ too much in pitch. "These beats," he writes, "are *powerful when their interval amounts to a half-tone or a whole tone, but weak and audible only in the lower portions of the scale, when it is equal to a third, and they diminish in distinctness as the interval increases.*"[‡] Here we see at once the reason why it is futile to attempt to determine consonances of a fifth or octave by the beats of two simple primaries—viz. that for these wide intervals the beats are imperceptible.

Let us now proceed to Prof. Tyndall's theory of consonance for *composite* sounds. Taking the octave C′, C″ or 264 : 528, he writes :—"With regard to the octave C′, C″, its two fundamental tones and their over-tones answer respectively to the following rates of vibration :—

		1	:	2	
Fundamental tone		264		528	Fundamental tone
Over-tones . .	1.	528		1056	
	2.	793		1584	
	3.	1056		2112	
	4.	1320		2640	
	5.	1584		3168	
	6.	1845		3696	
	7.	2112		4224	
	8.	2376		4752	
	9.	2660		5280	

"Comparing these tones together in couples, it is impossible to find, within the two series, a pair whose difference is less than 264. Hence, as the beats cease to be heard as dissonance when they reach 132, dissonance must be entirely absent from the combination. This octave, therefore, is an absolutely perfect consonance."[§] The same process is then applied to other intervals. For the *fifth* 264 : 396, the lowest difference between any two overtones being 132, the interval is "all but perfectly free from dissonance." For the *fourth*, 264 : 352, the least difference, 88, makes it "clearly inferior to the fifth." Similarly the *major third*, 264 : 330, with least difference 66, is "less perfect as a consonance than the *fourth*, and the *minor third* 264 : 316·8, with least difference 53, "inferior as a consonance" to all the previous intervals.[‖] In each case the "least difference" is precisely equal to the difference between the vibration-rates of the fundamental tones; so that, in spite of the array of figures, *nothing is added by this process to that employed by Prof. Tyndall to fix the consonances for simple tones.* Inasmuch, therefore, as that method has been proved erroneous, these determinations cease to have any validity.

Here, again, the neglect of the second condition for the production of audible beats is at the root of the error. Helmholtz does not only consider the differences of the vibration-rates of pairs of over-tones, but inquires also whether they are *within beating distance* of each other. In the former case alone can they become a source of dissonance. The interval of an *octave* is a perfect consonance, because every partial-tone of the higher sound coincides with one of those of the lower; and thus any slight deviation in pitch will produce beats between each adjacent pair.

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[*] T. p. 296.

[†] Tuning-forks produce, of course, *simple* tones.

[‡] T. pp. 296, 297.

[*] T. p. 297.

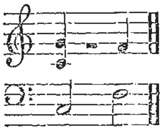
[†] Professor Tyndall's attempted explanation depending on *difference of phase* between the two primaries is at once refuted by the general principle that *difference of phase in partial-tones has no effect on quality*. See H. p. 193. A note and the octave above may obviously be treated as the ground-tone and first overtone of a composite sound.

[‡] H. p. 302.

[§] T. p. 299.

[‖] T. p. 299—301.

The interval of a *fourth* is less consonant than that of a *fifth*—not, as Prof. Tyndall represents it, on account of its 66 beats which, but a single octave below that in which he has placed it, ought to become the worst possible dissonance—but because the second partial-tone of the higher sound comes within beating distance (a whole tone) of the third partial-tone of the lower, as shown on the stave—



when the fundamental tones are written in minims, and the over-tones in crotchets.

I may as well notice that a diagram given by Helmholtz * to illustrate various degrees of dissonance, and copied by Prof. Tyndall, † is accompanied by the latter with an explanation giving a wrong idea of its meaning. The diagram, as explained by its author, is intended to represent to the eye the degrees of roughness attaching to intervals greater than one octave, and not exceeding two. Prof. Tyndall having evidently missed the remark of Helmholtz ‡ that C′, *not its octave* C″, "is to be the constant fundamental tone of all the intervals," has represented the diagram as "beginning with the unison C″—C″" and going up to the octave intead of beginning with the octave C′—C″ and going up to the double octave C′—C‴. The diagram as it stands in Prof. Tyndall's lecture is calculated to convey an impression as unlike its author's meaning as it is contrary to fact.

Trinity College, Cambridge, Feb. 26. SEDLEY TAYLOR

The Valuation of Liquid Town Sewage

THOUGH I consider it highly unbecoming for one member of a committee, charged with an important inquiry, to criticise publicly and in a controversial manner, views expressed by another member of that committee in regard to the subject it has to investigate, still some of the remarks made by Mr. Hope at the Society of Arts last Wednesday seem so unmistakeably to refer to the article which appeared in NATURE on the 23rd December last, that I feel constrained, as the writer of that article, to reply to them. The statement objected to by Mr. Hope was an expression, not of individual opinion, but of the fact—long accepted as beyond question—that the practical value of liquid town sewage as manure, that is to say, its value to the farmer, cannot be computed *solely* from the amount of manure material it may contain, and that, in forming such an estimate, the positive element afforded by chemical analysis must be controlled by the negative element introduced by extreme dilution, and varying under different local circumstances. This fact has been recognised by authorities too numerous to name, and so decisively, that Mr. Hope's assertion as to the value of the ammonia in sewage being affected only in a very minor degree by the amount of water mixed with it, seems to have no other merit than that of being "sensational." I am at a loss to conceive what ground Mr. Hope could have for objecting to the statement that "it is a great mistake, and likely to prove a very ruinous one," to estimate the value of dilute sewage by calculation solely from the amount of manure material it may contain. Yet this is what Mr. Hope characterises as a "strange paradox." Why it has puzzled him, as he admits, I will not stop to inquire; but I must protest against his representing "the obligation of applying water to crops at all times of the year, whether they want it or not," as having been one of the reasons given for the statement he objects to. In doing that he has at least fallen into a great error, and he has at the same time evaded the point to which attention was directed in the article, viz., the agricultural difficulty attending the "continuous daily application of sewage to land." That is a difficulty not to be disposed of *ex cathedra*—it would obtain whether the land destined to receive sewage were under crops or lying fallow. In the one case the application of sewage might be inadmissible during great part of the year; in the other case the land under fallow would be unproductive meanwhile. Indeed the need for applying sewage to fallow land, which Mr. Hope seems to suggest, would enhance the difficulty of disposing of sewage by irrigation, since it would involve the want of a still larger area of land for its reception, day by day throughout the year. Such a mode of application might well necessitate an area of twenty-five acres for every 100 persons, and that necessity, if it existed, would be, I imagine, a very serious matterin the case of many towns.

I will not attempt to occupy your space by considering the question whether leaving land under fallow is to be regarded as a feature of progress in agriculture; nor will I venture an opinion as to whether water be the "best dung-cart," further than to express my surprise that, in regard to this question, Mr. Hope should have recourse to a chemist's opinion while declaring that its decision is not within the province of the chemists.

The case put by Mr. Hope, with an air of anticipatory triumph, of a man who applies to his land an excessive and useless amount of manure, seems to me an exact parallel to the use of liquid sewage in many instances, for whether it be the fancy or the folly of the farmer, or some other circumstance, which impels him to use manure in such a way that the possible effect cannot come up to the amount of manure applied, I should imagine it to be obvious that, to the user, the value of the manure must be gauged by the practical effect likely to be realised. I should expect this view to be appreciated even by the bucolic intellect which Mr. Hope seems disposed to contemn.

As stated by the chairman at the Society of Arts meeting, the views held by Mr. Hope on the general subject of town refuse are clear and precise. There is no doubt what those views are, but it is not my intention to enter upon any discussion of them. At the same time, as a member of the British Association Committee, and having individually entertained the expectation that Mr. Hope's co-operation would be of material service in the inquiry it has to make, I cannot avoid expressing my regret that he declares himself a partisan of one particular solution of the town-refuse problem. There are, probably, few questions of the day which demand more careful and impartial consideration than this one—few that less admit of being dealt with for the promotion of a project at any price. For my own part, therefore, I deem it a misfortune that the value of Mr. Hope's well-known ability and extensive knowledge of this subject should be limited by his avowal of a foregone conclusion.

February 28. BENJAMIN H. PAUL

Weeds in Newly Turned Ground

FROM a recent address of Mr. Bentham, the President of the Linnæan Society, it would appear to be still uncertain whether the weeds which appear spontaneously on ground which has been newly turned over, spring from seeds hidden in the ground, or from seeds accidentally carried on to the new surface. Could not this question be decided by a simple experiment, namely, by turning over some suitable ground and covering parts of it by gardener's glass frames, so as to prevent the importation of any seeds? So far as the weeds are the same both within and without the frames, it is certain that they must spring from seeds previously contained in the earth. It is true that there will be a difference of temperature beneath the glass and in the open air, but it would not prevent us from learning what seeds are really contained in the earth. A frame covered with fine muslin would serve instead of glass if the muslin be fine enough to prevent the passage of any seeds.

J.

Skeleton Lectures on Science

I HOPE you will allow me, through your valuable pages, to suggest what I have no doubt would be a most powerful and successful means for promoting scientific knowledge throughout the length and breadth of the land. There are hundreds of persons with the desire and a sufficient taste and general knowledge of science, who now devote themselves to penny readings, who would be too glad to give popular lectures on scientific subjects, if they could only be aided by *skeleton lectures*, and the loan, on moderate terms, of simple apparatus, diagrams, &c., to illustrate the same. It certainly would well remunerate any scientific instrument maker to loan out apparatus for such lectures.

Skeleton sermons are enormously in request, why not skeleton lectures on science?

W. L.

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* The second on page 292.

† Page 208.

‡ Page 291.